has for one of its objects to make known to the public of the two Americas the value of French general culture and practical instruction.

Dr. Charles E. Marshall, professor of bacteriology and hygiene in the Michigan Agricultural College, has accepted an appointment as director of the graduate school and professor of microbiology, at the Massachusetts Agricultural College. He will begin his new duties on September 1. The Graduate School of the Massachusetts Agricultural College is an outgrowth of graduate work started under the direction of Professor Charles H. Fernald nearly eleven years ago. Four years ago the school was organized with Professor Fernald as director, and since his retirement Dr. Henry T. Fernald has been acting director. During the existence of the school fifteen men have been given the degree of master of science, and seven, the degree of doctor of philosophy. At the present time sixteen men are enrolled as graduate students. It is the intention of the trustees to develop the activities of the school.

Dr. Raymond A. Pearson, recently Commissioner of Agriculture for the state of New York, has accepted the presidency of the Iowa State College of Agriculture at Ames. Dr. Pearson has been granted leave of absence for the summer and will visit agricultural colleges in Europe.

Dr. Thomas McCrae, A.B., M.D., Toronto, associate professor of medicine at the Johns Hopkins University, has been appointed professor of medicine in Jefferson Medical College, Philadelphia, to fill the chair vacant by the resignation of Professor James C. Wilson.

At the University of London Professor F. G. Donnan, F.R.S., has been appointed to the chair of general chemistry at University College, in succession to Sir William Ramsay. Dr. L. N. G. Filon, F.R.S., has been appointed to the Goldsmid chair of applied mathematics and mechanics to succeed Professor Karl Pearson, who resigned this chair on his appointment to the Galton chair of eugenics.

DISCUSSION AND CORRESPONDENCE

ARE HORNS IN SHEEP A SEX-LIMITED CHARACTER?

Arkell and Davenport in Science for March 8, 1912, answer this question in the affirmative on the basis of certain crosses which they have made between horned and hornless races. In doing so they call in question the authenticity of a statement made by me in a recent publication as follows:

> In merino sheep the male has well-developed horns, but the female is hornless; yet if the male is castrated early in life no horns are formed.

They comment thus:

> He gives no reference for the last statement; and in view of the variability of the horned condition in the males of the "merinos" the conditions of the experiments would have to be carefully considered before such a result could be accepted as settling the question of the dependence of horns in heterozygous males upon a secretion from the testis.

In reply to this criticism, I beg to say that I gave no authority for the statement in question because I can myself vouch for it. I grew up on a farm where Merino sheep were kept in considerable numbers. From my earliest recollection until I was 21 years old I saw the operation of castration practised each year on 50 or more ram lambs and its effects were perfectly familiar to me. The result is exactly that stated. If the male is castrated early in life, say within a month after birth, no horns develop. If castration has been delayed for two or three months, the horns begin to grow, but castration then promptly arrests their growth. I can recall but one exceptional case in the hundreds that came under my observation. In that case the horns continued to grow for some weeks. When this case was observed, the animal was caught and found to have been imperfectly castrated. A second operation caused cessation of the horn development. My father used registered Atwood Merino rams, and his ewes were pure bred. The ewes were regularly hornless as they are typically in this breed. See figures in Robert Wallace's (1907) "Farm Live Stock of Great Britain," p. 592. The males were as regularly horned, if not castrated. We usually

raised several of these (uncastrated) males each year and they were remarkably uniform in horn development. Compare again Wallace's figures, which are accurate, being photographs. Arkell and Davenport's statement concerning the "variability of the horned condition in the males of the Merinos" leads me to think they must refer to *grade* Merinos, certainly not to the pure bred ones.

To my mind the evidence is clear that in pure Merino sheep castration does prevent development of the horns, and I have no doubt that in other breeds also castration has similar though perhaps less conspicuous effects. In breeds which are horned in both sexes the males regularly have *better developed* horns than the females, and castrated males have smaller horns than uncastrated ones. See figures in Wallace.

If castration has the effect stated, the assumed nuclear *inhibiting* factor of Arkell and Davenport is quite superfluous. Their experimental results are fully in accord with those of Wood and are fully covered by the simple statement of Bateson that the horned character is "*dominant in males and recessive in females.*" Why this is so I have attempted to point out. Presence of the testicle is necessary for full horn development, in some breeds it is necessary for *any* horn development. Reasoning from the experimental results obtained in poultry it seems probable that injections of testicle extract into the female would cause increased horn development similar to that of the male. It would be interesting to know whether the testicle of all breeds would behave alike in this experiment. Whether the female sex gland acts as an inhibitor of horn development would be a wholly different question, yet one capable also of experimental solution.

To assume, as Arkell and Davenport do, that inhibiting factors present in X-chromosomes affect the horn development seems to me unwarranted, for the simple reason that neither inhibitors nor X-chromosomes are known to exist in sheep. That Guyer has recognized the existence of an X-chromosome in man has no very direct bearing on the question, but even Guyer's result is unconfirmed by Gutherz, who has reinvestigated the spermatogenesis of man upon exceptionally favorable material.

Arkell and Davenport reason thus:

> The results of the table [of crosses] accord very closely with expectation, so that we are justified in concluding that an explanation of the results like that we offer is the correct one.

But Bateson's explanation accords also; wherein lies the superiority of the new one offered? To establish the probable correctness of a hypothesis it must be shown that no other hypothesis accords with observed facts equally well. Has this been shown in the case before us?

Consider how one unproved hypothesis has been added to another. First it is assumed that in hornless animals a gene for horns has either been lost or is inhibited. It is equally probable that no gene has been lost and that nothing is inhibited. Secondly, it is assumed that one inhibitor is inferior to one horn-gene in power, but that two inhibitors surpass one horn-gene, yet two inhibitors are themselves overpowered by two horn-genes; without all three of these ungrounded assumptions of the relative valency of imaginary genes the explanation fails altogether. Further, it is assumed that the female is capable of carrying two inhibitors, but the male only one. And finally when this colossal structure of hypothesis encounters one well-known physiological fact, the result of castration, that fact is calmly brushed aside. Is this a desirable extension of Mendelian interpretation?

W. E. CASTLE

March 13, 1912

THE MOTH OF THE COTTON WORM (ALABAMA ARGILLACEA HUBN.)

TO THE EDITOR OF SCIENCE: In connection with the notices appearing in SCIENCE (October 16, November 10 and December 29), concerning the moth of the cotton worm and the destructive work of the caterpillar on cotton, a note from Missouri may be of interest.

During the fall this moth was present in great numbers in various parts of the state.